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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Acting, predicting and intervening in a socio-hydrological world" *by* S. N. Lane

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General Comments

The thesis of this paper seems to be that all hydrological modelling should be considered as a socio-hydrological practice because it takes place within a particular social (or political) context. As such this is an interesting paper that is relevant to the hydrological sciences community that HESS serves. However, I found this paper quite challenging to read, particularly the first 3 sections, and I believe that it requires some effort to improve the clarity of the message, to allow the reader to get the most from it.

It should be pointed out that I am reviewing this paper as an interested hydrologist (possibly the main audience of the paper) with very little experience of the STS litera-

ture, and so it may be wise to invite responses from more than one reviewer from the STS community.

This paper seems to aim for two target audiences, the first being the social scientists that are interested in this from a theoretical viewpoint; the 'science-technology studies' (STS) discipline, and the second being hydrological scientists themselves. Writing for two different audiences is of course a challenge, and I think this article would benefit from a more careful consideration of how, or perhaps even if, this can be achieved. If the aim of this article is to convince hydrological scientists to take a more reflexive view on their own practices and consider the context in which they are working, I'm concerned that the abstract and introduction are more relevant to the first target audience and so would lose the hydrological scientist audience at the first hurdle, and using a lot of STS terms throughout the article but without including a layman's definition might only serve to alienate the hydrological audience further.

Additionally, I think the terms hydrology / socio-hydrology / hydrological / social-hydrological seem to be used interchangeably throughout the article and without adequate definition, also with phrases such as 'how we practice', who 'we' is should be well defined. Is every hydrologist a socio-hydrologist, or would every hydrologist identify themselves as one? I don't think so, and nor do I think they should, but that doesn't mean that every hydrologist shouldn't consider the socio-hydrological context or implications of their work.

I also think that the exact definition of socio-hydrology isn't particularly clear, it relies on the reader having read Sivapalan and Blöschl (2012), whereas I think because it is the main focus of the paper it would help for the author to define how he uses it within this paper.

The author in places switches from an 'opinion paper' style to that more like a review article. I believe that the value of the paper is as the former and care should be taken to ensure that the reader is not mislead into believing that the paper provides an all-

encompassing evaluation of the literature; this can be resolved by providing a clearer introduction to each section that states its focus.

Specific Comments

Section 2. For the hydrological science community it would be helpful to define what a 'constuctivist account' is, and also who the science-technology studies community are.

Line 9. Sentence beginning "Each of the three accounts": if these accounts are well developed could you point the hydrologist reader to a relevant text that might provide a more comprehensive summary?

Line 14: "Equally, there is equally evidence" perhaps needs changing

Section 2.1 This section aims to provide a brief introduction to STS for the hydrology community, but I am concerned that there are parts of this section that are irrelevant and don't act to guide the reader towards the concluding paragraph, and equally, that while Latour is perhaps one of the dominant voices in the STS literature, there may be other voices lacking from the discussion. This is something that is difficult for me to evaluate as a hydrologist. Nevertheless, this section is quite descriptive, summarising the literature rather than drawing from it, and so would perhaps benefit from being more concise and focussed throughout.

Page 10665 and 10666: Description of Latour and Woolgar book, Line 4: "claim to scientific authority": Is this statement from the Latour and Woolgar book still? I think it is fine to refer to this if it is included as part of a summary of the book, but I think there is a much larger literature post 1979 on scientific authority that could be discussed e.g. with respect to climate change and uncertainty.

Section 2.2 Science in decision-making

Is this section meant to represent the STS view of science in decision-making? If this is the case then this section would benefit from a short introduction that describes (and limits) its focus. If it is meant to represent a wider view of science in decision-making

then it lacks a lot of detail and misses significant literature, e.g. from Nick Pidgeon, Roger Pielke Jr, Daniel Sarewitz etc.

I would also suggest that it is important to describe what is meant by 'how science is used in decision-making' in this article, as it can take on different meanings other than what is perhaps intended by the author, in the psychological literature for example, or when addressing communication of science.

This section in general would benefit from a more considered structure, something that might perhaps fall into place once an introduction had been written. For example, this section is meant to be the 'second constructivist account', and on p10668 line 21 it says three objections will follow, with line 22 listing the first, then it gets a bit lost with a second (p10669 line 5), a third (p10669 line 10), another second (p10669 line 22) and another third (p10670 line 7). Untangling these nested points would be helpful! My own preference would be to split the section into two, with the first focussed on uncertainty, and the second focussed on the issues tackled by p10669 line 22 onwards.

To aid the hydrologist reader the uncertainty parts could perhaps be better defined with respect to terminology used commonly in the hydrological literature; epistemic, aleatory and Knightian uncertainties, described nicely by Beven (2008). Certainly it would help to describe what kind of uncertainties are being referred to in the text, although perhaps the focus is mainly on epistemic uncertainties or ignorance rather than anything aleatory or stochastic? Of course in the case of meteorology and increasingly hydrometeorology uncertainty information such as the frequently communicated 'probability of precipitation' is not a manifestation of ignorance rather an informed quantification of uncertainty in a chaotic system, and the issues surrounding decision-making from this information are therefore different (see for example Rebecca Morss or the literature surrounding decision making from uncertain hurricane track forecasts).

Page 10669 Lines 1-4. While not specifically related to the context of this section, I think given the overall theme of this article it is important not to use hydrological

examples without some kind of description of the socio-hydrology behind them. The description of inundation patterns that are '80 to 90% accurate' is reported in such a way because of perhaps a) a history of using this kind of statistic, e.g. in previous literature, and / or b) because there is a demand from end-users, and not necessarily because this the 'best' way of evaluating or communicating model performance (see Stephens et al. 2012 sec2.4). For example, organisations such as the UK Environment Agency might prefer it to be reported in such a way perhaps related to organisational perceptions of what can be understood by non-experts. Additionally, modellers are perhaps under pressure to demonstrate the ability (rather than lack of) of their models and using a percentage value makes a good headline figure. This perhaps ties in with a point made in the following section (from Lahsen, 2005), about how modellers are often ineffective at identifying flaws in their own models or are tied into specific practices even where the processes are not effective. The reporting of inundation model accuracy is a very good example of this, as it has been shown that the binary pattern performance measures that the percentages listed in this article are produced from are not fit for purpose (see Stephens et al. 2013). Additionally, I would argue that the problem of flood events not matching their predicted outline is largely due to the magnitude of a particular flood event not being identical to the event that has been modelled (e.g. 1 in 100 year), rather than due to model deficiencies.

Section 2.3 Certified and non-certified hydrological experts

I think the author makes an interesting point about certified and non-certified experts, but I am uncomfortable with the division of groups into 'scientist' and 'public' roles; this is quite a generalisation, where do policy-makers fit with this? The author writes about 'the linear model of science into policy', in relation to the 'deficit model', but I think the distinction needs to be made between 'public education' [i.e. to a general audience for perhaps a general purpose] and dissemination to 'policy makers' [i.e. a specific and perhaps expert audience for a specific purpose]. I also think the 'linear model of science into policy' needs more of an introduction; does this linear model come from

Sturgis and Allum (2002)?

I'm also not sure how the point about 'being disabled from reasonable arbitration' on page 10674 is relevant to the rest of the section about certified / non certified expertise. I find the rest of this section that continues onto page 10675 difficult to follow. In the last sentence I am unsure who 'we' is referring to; socio-hydrologists or hydrologists? And should it be uncertified or certified experts?

Section 3.

The introduction to this section is well written and perhaps could serve as an example to the other sections?

Section 3.6

I think more could be said to discuss the implications of taking something from a lab based or empirical work and then applying it to a model-world that is discretised in a particular way, as grid squares for example. The concept of 'effective parameterisation' should also be discussed, as this is the terminology used by Keith Beven etc. Line 10688 L3 Stephens et al. (2012) also demonstrates the problem of calibrating inundation models using an uncertain satellite-observed flood extent. It is interesting also that whether the 'optimum parameter set' is deemed physically correct or not is often used as a diagnostic indicator of whether there is something wrong with the model in terms of additional uncertainties (which could be in the boundary condition, the historical / observed data or elsewhere). Conversely, if the parameter set 'looks about right', i.e. they're in the right ballpark for what would be expected from the terrain then this potentially leads to overconfidence in the model (I guess there is an assumption that a physically realistic optimum friction value means that the model will be less affected by non-stationarity), whereas it could be getting things right for the wrong reasons. Again this is perhaps one of those circumstances where a discipline adopting particular practices makes the modeller ineffective at spotting flaws in the model.

I think an extension to Section 3 should perhaps be written to incorporate ideas that are introduced in the following section; those that relate to the importance of networks within which science is practiced. In particular, the author has enough experience of hydrological modelling in the UK to draw on the differences between how model calibration (or sensitivity analysis) is carried out within academic circles and within the consultancies that provide flood modelling services to the Environment Agency (for example). Additionally, James Porter's thesis (Section 6.6 'Floodplain Frictional Resistance: Why All the Debate?', available from the author), provides a good discussion on the topic. Those consultancies carry out this work within particular regulatory confines; perhaps on how many simulations should be run with differing values of Manning's n to satisfy the requirements for sensitivity analysis. How these regulations have been defined would provide an interesting case study and example for this paper, even if it is specific to the situation in the UK. Additionally the contrast with climate modelling, that doesn't have the same reliance on the private sector for their predictions (and therefore perhaps the regulations are different) is quite interesting.

Section 4.

Sp. Some citations are to Knuuttila and others to Knuttila.

Section 5.

P10694 Line 12. Would help to expand on why hydrological science is inherently controversial.

Section 5.5. The example of 'Environmental Competency Groups' provides a really valuable lesson in how best to engage stakeholders is flood risk management.

Section 6.

P10704, line 2. Should this be 'to forget' not 'to forgot'? Potential Further References:

Beven, Keith. Environmental modelling: an uncertain future?. Taylor & Francis, 2008.

Cash, David W., Jonathan C. Borck, and Anthony G. Patt. "Countering the loading-dock approach to linking science and decision making comparative analysis of El Niño/Southern Oscillation (ENSO) forecasting systems."Science, Technology & Human Values 31.4 (2006): 465-494.

Morss, Rebecca E., et al. "Flood risk, uncertainty, and scientific information for decision making: lessons from an interdisciplinary project." Bulletin of the American Meteorological Society 86.11 (2005): 1593-1601.

Pielke, Roger A. The honest broker: making sense of science in policy and politics. Cambridge: Cambridge University Press, 2007.

Regnier, Eva. "Public evacuation decisions and hurricane track uncertainty."Management Science 54.1 (2008): 16-28.

Sarewitz, Daniel, Roger A. Pielke, and Radford Byerly, eds. Prediction: science, decision making, and the future of nature. Island Press, 2000.

Stephens, E. M., et al. "The impact of uncertainty in satellite data on the assessment of flood inundation models." Journal of Hydrology 414 (2012): 162-173.

Stephens, Elisabeth, Guy Schumann, and Paul Bates. "Problems with binary pattern measures for flood model evaluation." Hydrological Processes (2013).

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